Response to Anonymous Referee #4

We thank Anonymous Referee #4 for the time and effort devoted to the review of the manuscript. Below, we reproduce the reviewer's comments and address their concerns in each case. The reviewer's comments are copied below in regular font, with our responses in orange. We are responding to this review a long time after it was published on the *Biogeosciences Discussion* online forum because we had to submit the companion paper by Caffin et al. (this issue), cited in this article, before the closure of OUTPACE's special issue on December 31, 2017.

Summary Statement

Caffin et al. constructed a nitrogen budget for three stations in the western tropical Pacific Ocean by quantifying N2 fixation, NO3 diffusion, atmospheric deposition, and PN export. Overall, the study seems to be well-conducted, arguments are supported by data, and the paper is well-cited. There are some relatively minor issues, mostly with the presentation, as described below. The manuscript requires a thorough editing to correct awkward word choices, punctuation errors, and confusing text. The main point I found that was missing from the paper was a definition of the system being studied. When the authors attempted to describe the system and site selection choices, the text was confusing and too vague, so this area of the paper could be improved. Some additional details are also missing from the methods and should be included. The conclusions section fell a bit flat and could be bolstered by putting the study findings into a better context relative to filling information and data gaps and describing the overall importance of the study results for our understanding of the global ocean. None of these issues represent serious barriers to publication, in my view, and only minor revisions are needed.

Again, we thank Anonymous Referee #4 who highlighted some 'relatively minor issues', 'mostly with the presentation', and we will respond point by point to the comments. Moreover, the text has been corrected by an English native speaker. We have included missing additional details regarding the method and opened up the conclusions with a view to making good the information gaps to enhance our understanding of the WTSP, that is a hot spot of N_2 fixation, and our understanding of extensive areas of the oligotrophic ocean.

Specific Comments

Abstract

Overall, I found the Abstract was confusing. There is no clear direction, and the text jumps around from topic to topic without any clear context for the study or results. The concluding sentences do not place the study findings into any sort of importance relative to information and data gaps that we have for the WTSP (or other areas of the oligotrophic ocean). Why is the disequilibrium and apparent N accumulation important to describe?

P1, Lines 21-22 — Confusing sentence. Rewrite for clarity.

The sentence "We performed N budgets at three stations in the western tropical South Pacific (WTSP) Ocean during austral summer conditions (Feb. Mar. 2015) and quantified all major N fluxes both entering the system (N_2 fixation, nitrate eddy diffusion, atmospheric deposition) and leaving the system (PN export)." has been

rewritten as follows: "We performed N budgets in the photic layer of three contrasting locations in the western tropical South Pacific (WTSP) during austral summer conditions (Feb. Mar. 2015) and quantified all major vertical N fluxes both entering photic layer (N_2 fixation, nitrate turbulent diffusion, atmospheric deposition) and leaving the photic layer (PN export).

P1, Lines 24-25 — Is there more information on these locations, other than just DCM, that could be presented to give the reader a better idea of what these sampling locations are like?

We have rewritten the Abstract as suggested and indicate the strong nitracline depth differences, allowing the reader to understand that we sampled a strong oligotrophic gradient. The depth values were not indicated in the Abstract because they do not represent major findings.

New Abstract:

We performed nitrogen (N) budgets in the photic layer at three contrasting stations representing different trophic conditions in the western tropical South Pacific (WTSP) Ocean during austral summer conditions (Feb. Mar. 2015). Using a Lagrangian strategy, we sampled the same water mass for the entire duration of each long duration (5 days) station, allowing us to consider only vertical exchanges for the budgets. We quantified all major vertical N fluxes both entering the photic layer (N_2 fixation, nitrate turbulent diffusion, atmospheric deposition) and leaving the system (particulate N export). The 3 stations were characterized by strong nitracline and contrasted deep chlorophyll maximum depths, which was lower in the oligotrophic Melanesian archipelago (MA, stations LD A and LD B) than in the ultra-oligotrophic waters of the South Pacific gyre (SPG, station LD C). N_2 fixation rates were extremely high at both LD A (593 ± 51 µmol N m^{-2} d^{-1}) and LD B (706 ± 302 µmol N m^{-2} d^{-1}), and the diazotroph community was dominated by Trichodesmium. N_2 fixation rates were lower (59 ± 16 µmol N m^{-2} d^{-1}) at LD C, and the diazotroph community was dominated by unicellular N2-fixing cyanobacteria (UCYN). At all stations, N_2 fixation was the major source of new N (> 90 %) before atmospheric deposition and upward nitrate fluxes induced by turbulence. N2 fixation contributed circa 13-18 % of primary production in the MA region and 3 % in the SPG water and sustained nearly all new primary production at all stations. The e-ratio (e-ratio = particulate carbon export / primary production) was maximum at LD A (9.7 %) and was higher than the e-ratio in most studied oligotrophic regions (~1 %), indicating a high efficiency of the WTSP to export carbon relative to primary production. The direct export of diazotrophs assessed by qPCR of the nifH gene in sediment traps represented up to 30.6 % of the PC export at LD A, while their contribution was 5 and < 0.1 % at LD B and LD C, respectively. At the three studied stations, the sum of all N input to the photic layer exceeded the N output through organic matter export. This disequilibrium leading to N accumulation in the upper layer appears as a characteristic of the WTSP during the summer season, although the role of zooplankton in export fluxes should be further investigated.

Introduction

P3, Lines 1-8 — The authors need to define the "system" they are talking about. What are the boundaries of the "system"?

Reviewer #4 is right and we have specified that our systems were the photic layer.

Are sediments included?

Of course, the sediments were not included in our open ocean areas. It should be clearer now with the previous definition of the systems

What does "...with an adequate time frame under contrasting diazotroph communities' composition" mean?

The adequate time frame is required to link export and production. The contrasting diazotroph communities' composition means if the diazotrophs are dominated by *Trichodesmium* or UCYN.

The sentence: "Studying the impact of N_2 fixation on PN export in the ocean and the relative role of each diazotroph group in this process are technically challenging. It requires the measurement of all major N fluxes both entering the system (N_2 fixation, nitrate (NO_3^-) eddy diffusion, atmospheric deposition) and leaving the system (PN export) with an adequate time frame under contrasting diazotroph communities' composition." has been rewritten as follows: "Studying the impact of N_2 fixation on PN export in the ocean and the relative role of the main diazotrophs is technically challenging. It requires the measurement of all major N fluxes both entering the photic layer (N_2 fixation, nitrate (NO_3^-) eddy diffusion, atmospheric deposition) and leaving the photic layer (PN export) with an adequate time frame (i.e. linking production and export). In addition, it has to be performed under contrasting situations, when either Trichodesmium or UCYN was dominating the diazotroph community, to assess the potential role of each diazotroph group."

Does "the same water mass" mean that horizontal water movement is not present/considered?

There were little horizontal movement in the low horizontal advection areas chosen, and our strategy was to sample along the flow, thus minimizing horizontal advection fluxes. Note that fluxes need gradients of properties and they are really low horizontally in open ocean areas, particularly as station locations were specifically chosen in low horizontal current areas.

Are there processes occurring within (or beyond) the boundaries of this "system" that could confound the approach?

As mentioned, vertical movements of zooplankton may probably play a significant role but are difficult to quantify.

P3, Lines 9-16 — The authors should provide more information on the trophic gradient and how 'oligotrophic' and 'ultra-oligotrophic' are defined. What are the physical factors causing the gradient?

The paragraph: "The WTSP Ocean has recently been identified as a hot spot of N_2 fixation, harbouring N_2 fixation rates >500 µmol N m^{-2} d^{-1} (Bonnet et al., 2017). The region covered by the OUTPACE cruise is characterized by trophic and N_2 fixation gradients (Moutin et al., 2017), with oligotrophic waters characterized by high N_2

fixation rates (631 ± 286 µmol N m^{-2} d^{-1}) mainly associated with Trichodesmium in the western part (i.e. within the hot spot around Melanesian archipelago waters, hereafter named MA), and ultra-oligotrophic waters characterized by low N_2 fixation rates (85 ± 79 µmol N m^{-2} d^{-1}) mainly associated with UCYN in the eastern part (South Pacific gyre, hereafter named SPG) (Bonnet et al., this issue; Stenegren et al., this issue).” has been rewritten as follows: “The WTSP Ocean has recently been identified as a hotspot of N_2 fixation, including N_2 fixation rates >500 µmol N m^{-2} d^{-1} (Bonnet et al., 2017). The region covered by the OUTPACE cruise encompasses contrasting trophic regimes characterized by strong differences in top nitracline depths, from 46 to 141 m (Moutin et al., this issue), and representing a large part of the oligotrophic gradient at the scale of the world Ocean (Moutin and Prieur, 2012; their Fig. 9). The westward oligotrophic waters are characterized by high N_2 fixation rates (631 ± 286 µmol N m^{-2} d^{-1}) mainly associated with Trichodesmium (i.e. within the hotspot around the Melanesian archipelago waters, hereafter named MA), and the eastward ultra-oligotrophic waters (in the eastern boarder of the South Pacific gyre, hereafter named SPG waters) are characterized by low N_2 fixation rates (85 ± 79 µmol N m^{-2} d^{-1}) mainly associated with UCYN (Bonnet et al., this issue; Stenegren et al., this issue)”

The strong ultra-oligotrophy of the South Pacific gyre has previously been described in the BIOSOPE special issue (https://www.biogeosciences.net/special_issue19.html).

P3, Lines 17-19 — The points of focus are great, but were there hypotheses to be tested? Why was it important to focus the study on these three points? What information/data gaps were being filled by conducting the study?

The sentence is: “In the present study we focus on (i) the contribution of N_2 fixation to new N inputs in the WTSP during the summer season; (ii) the coupling between N_2 fixation and export; and (iii) the equilibrium versus disequilibrium between N_2 fixation and particulate N export in the WSTP.” We consider that the plan of our paper has to be placed at the end of the Introduction. It is important to focus the study on these three points because as stated by Reviewer #4, the 3 'points of focus are great'. The hypotheses we tested were “Is nitrogen fixation a predominant flux in the photic layer of the WTSP?”, and “Are we able to link specificity in export with the dominant diazotroph groups?”

We provided the first N budgets following a Lagrangian strategy allowing confirmation of the predominant role of nitrogen fixation in the WTSP, as well as a large dataset of new data from the poorly studied South Pacific Ocean (http://www.obs-vlfr.fr/proof/php/outpace/outpace.php).

Methods

P3, Line 27 - P4, Line 14 — There are not enough details on the 3 criteria for site selection. What were the parameters of “local minima of surface current intensity” used to determine if conditions were suitable? How much surface current was considered acceptable? Were deeper currents considered? How was trophic status defined? In terms of chlorophyll or something else? If so, what were the thresholds used for oligotrophic, ultra-oligotrophic, etc.?

We were looking for local minima of surface current intensity in order to find adequate locations for our Lagrangian strategy, and we found them. The oligotrophic gradient was

necessarily sampled when we crossed 4000 km of the WTSP from the Melanesian archipelagoes to the South Pacific gyre. We understand the reviewer's comment about the choice of sampling sites and we apologize for this. The question is so important that a specific paper was devoted to that question by Alain de Verneil et al. (this issue), and unfortunately, it was not available at the time of Reviewer #4's response. The paper is now available here: https://www.biogeosciences-discuss.net/bg-2017-455/).

P5, Line 3—Was the chlorophyll fluorescence sensor calibrated to simultaneous samples analyzed for chlorophyll using more conventional extraction techniques?

The chlorophyll fluorescence sensor was calibrated prior to the cruise and post-calibration was conducted using all HPLC measurements undertaken during the cruise. Unfortunately, the relationship was not good, indicating that we sampled really different communities along the 4000 km transect. Nevertheless, considering the poor relationship between chl a and biomass, particularly with depth in the SP Ocean (Duhamel et al., 2007), we considered that our pre-calibrated sensor was good enough to represent well to be sufficiently representative of the oligotrophic gradient sampled.

Duhamel, S., T. Moutin, F. Van Wambeke, B. Van Mooy, P. Rimmelin, P. Raimbault, and H. Claustre. Biogeosciences, 4, 941-956, https://doi.org/10.5194/bg-4-941-2007, 2007

P5, Lines 4-7—Were nutrient samples analyzed immediately, or filtered and stored for analysis later (if so, provide details on procedures used), or not filtered at all...? Why wasn't ammonium included in the nutrient measurements?

Samples were filtered and analyzed both immediately and in the laboratory after poisoning. Ammonium was measured on board and data are available but we focus here on new N budgets in the photic layer and were not interested in regeneration.

P5, Lines 9-17 — It is unclear where the "associated N uptake" part of this section is evaluated. More details are needed describing sample handling and analyses for the PP incubations.

The "associated N uptake" corresponds to the N-derived PP (N-PP) mentioned in the paper. To be clearer, we have rewritten the sentence as follows: "A N-derived PP (N-PP), i.e. the associated N uptake, was obtained…"

Further details have been added in the final version concerning PP incubation methods: "PP was measured in triplicate using the ^{14}C tracer method (Moutin and Raimbault, 2002). Samples were incubated in 150 mL polycarbonate bottles on the in situ drifting production line…"

P5, Lines 19-22—More details are needed on the aerosols sampling, especially since the reference given for the method is only a submitted paper. Is there a reason why ammonia was not included in the atmospheric deposition measurements?

We completely agree with this remark. We have thus completed this section in collaboration with Cécile Guieu, who performed the measurements. Thus, Cécile Guieu will be added as co-author of the paper in its final version.

The new version of the section is:

"N atmospheric deposition (NO_3^- and NO_2^- (nitrite), hereafter called NOx) was quantified along the transect after dissolution of aerosols collected continuously during the transect, as described in Guieu et al. (in rev.). Briefly, the sampling device, designed to avoid ship contamination, was installed at the look-out post in the front of the ship, collected aerosols at ~20 L min^{-1} onto polycarbonate, 47-mm diameter, 0.45-µm porosity (previously acid-cleaned with a 2% solution of HCl (Merck, Ultrapur, Germany) and thoroughly rinsed with ultra-pure water and dried under a laminar flow bench and stored in acid-cleaned Petri dishes). Dissolution experiments to determine NOx released in surface seawater after deposition were performed on board using acid-cleaned Sartorius filtration units (volume 0.250 L) and filtered surface (5 m) seawater. Each sample was subjected to two contact times: the first contact was at one minute, and the second contact was at 24 hours. NOx was analyzed using 1-m long Liquid Waveguide Capillary Cells (LWCC) made of quartz capillary tubing, following the protocol described in Louis et al., 2015. An extrapolated NOx from dry deposition was estimated on the basis of a deposition velocity of submicronic particles (0.4 m s^{-1}; Vong et al., 2010)."

P5, Line 29 – P6, Line 3 — Very confusing sentence. Rewrite for clarity.

The sentence has been rewritten as follows: "It has been previously shown that the bubble method was potentially underestimating N_2 fixation rates (Großkopf et al., 2012; Mohr et al., 2010) compared to methods consisting in adding the $^{15}N_2$ as dissolved in a subset of seawater previously N_2 degassed (Mohr et al., 2010). This underestimation is due to incomplete equilibration of the $^{15}N_2$ gas with surrounding seawater. However, other studies did not find any significant difference between the two methods (Bonnet et al., 2016b; Shiozaki et al., 2015)."

P6, Lines 7-10 — How were dissolved gas samples transferred from the bottles to Exetainers? Kana et al. (1994) does not cover 15N2 measurements/analyses using MIMS. Is there another citation for the 15N2 analyses using MIMS?

The dissolved gas analyzed was contained in the seawater, thus 12 mL of seawater was rapidly sub-sampled with an eyedropper from the bottles to the Exetainers to avoid contamination. In their study, Kana et al. (1994) measured dissolved N_2, O_2 and Ar in water using MIMS. For that purpose, they detected masses 28, 32 and 40, corresponding to N_2, O_2 and Ar, respectively. In our study, we detected masses 28 and 30 corresponding to $^{14}N_2$ and $^{15}N_2$ respectively.

P7, Lines 1-3 — perhaps add "and" before daily? Something is missing in this sentence.

The sentence has been rewritten as follows: "The time series of the NO_3^- diffusive flux was calculated using an hourly temporal interpolation of Kz over the entire duration of each LD station. Also, daily averages and 5-day averages were computed."

P7, Lines 12-15 — Define "swimmers". PP was previously defined as primary production, so also using it for particulate phosphorus is confusing.

Swimmers correspond to zooplankton, as we considered in a first approximation that all zooplankton were alive. We have changed the "PP" of particulate phosphorus to "POP" to avoid confusion with primary production, and because in open ocean areas,

particulate phosphorus and particulate organic phosphorus are identical, and this is the term mostly used.

Results & Discussion

P9, Lines 24-27—What was the integration depth used for these rates? It is odd to see areal rates reported for a depth-integration that apparently does not include sediments.

The rates were integrated from the surface to 20 m below the last measurement (considered as nil) using the trapezoidal method for integration according to the classical JGOFS protocol. This allows us to be consistent within the three LD stations which presented different photic layer depth. The same depths were used for the calculation of integrated N_2 fixation rates over the photic layer.

P9, Line 30—Is there really a strong contrast between LD A and the other two stations given the very large variability around the mean at LD A (24.4 ± 24.4)?

The mean NO_3^- flux at LD A was around 3.5 and 5 times higher than at LD B and LD C, respectively, that is it is clearly contrasted. The wide variability around the mean is characteristic of the pulse that was observed at the end of the LD A survey, and which strongly increased the mean Kz at LD A. The differences in estimated fluxes between the stations is mainly attributable to differences in Kz between the stations. The gradients of concentration varied little between stations (table 1). The turbulent diffusion contrast is a robust result despite the variability of the signal (Kz and therefore the flux). This is explained by the fact that shear instability, the dominant process triggering turbulence at long stations, is more intense in LDA because of higher shear, by a factor of about 3. Thus, the strong contrast between LD A and the other two stations is confirmed.

P11, Line 11 — Perhaps the authors should use LD-A, LD-B, and LD-C to denote their stations, instead of LD A, LD B, and LD C. There have been a few cases like here (LDA) where the site abbreviations have not been consistent.

The references LD A, LD B and LD C have been kept in the interests of consistency with all the papers of the OUTPACE special issue.

P12, Lines 3-23 — I found this narrative confusing. Perhaps the authors could streamline this text to focus it on the most important points?

In accordance with this remark, we have 'streamlined' the text has much as possible. However, much important information is mentioned in this section that it is not possible to delete. We explain that we observed an unreported high contribution of N2 fixation in this region compared to low atmospheric deposition and vertical turbulent diffusion. In addition, we discuss both of those inputs in a general context, and therefore think this discussion is important and has its place in this section.

The text has been rewritten as follows:

"Extrapolated NOx deposition from the atmosphere during OUTPACE (range: 0.34 – 1.05 µmol m^{-2} d^{-1}) was one order of magnitude lower than predicted with major uncertainties by global models that include wet and gas deposition for that region (Kanakidou et al., 2012). Our flux could be an underestimation as it represents dry

deposition and gas and organic forms were not measured. At global scale and depending on the location, organic nitrogen could represent up to 90 % of N atmospheric deposition (Kanakidou et al., 2012). Even if we double our estimated deposition flux, atmospheric deposition remained low (< 1.5 %) and consequently represented a minor contribution of the new N input (Table 4). This negligible contribution of atmospheric input to the overall N budget (less than 1.5 %), therefore implies a major contribution of other terms, such as N2 fixation.

Then, NO_3^- input by vertical diffusion appeared as the second source (1 to 8 %) of new N at the three stations. This contribution was lower than in previous studies in other oligotrophic regions (Table 5) where NO_3^- input by vertical diffusion contributes ~ 18 % of new N in the Indian South Subtropical Gyre (Fernández-Castro et al., 2015), and ~ 50 % in the Tropical North Atlantic (Capone et al., 2005). In most studies (Fernández-Castro et al., 2015; Moutin and Prieur, 2012; Painter et al., 2013), an average Kz value is used (i.e. averaged over the cruise, over a station or over depth) to determine NO_3^- input by turbulence in the photic layer. In this study, we performed high frequency direct measurements of Kz and highlighted the importance of turbulent event pulses on diffusive NO_3^- input. Using a constant Kz of 10^{-5} m^2 s^{-1} at the 3 stations decreases the NO_3^- input down to 22.9 µmol N m^{-2} d^{-1} at LD A and increases NO_3^- input up to 19.9 and 25.5 µmol N m^{-2} d^{-1} at LD B and LD C, that is 2.7 and 4.8 times higher than using a high frequency Kz for the latter two stations. The contrasted NO_3^- input observed at the three stations results from the high variability in turbulence along the west-east transects (Bouruet-Aubertot et al., this issue). Thus, using a constant Kz removes the contrasted NO3- input between the 3 stations (~ 4 times higher at LD A than at LDB and LD C). Consequently, using average Kz values for the diffusive flux computation can lead to significant bias. In our study, NO_3^- input was calculated at the top of the nitracline. Painter et al. (2013) have demonstrated the variability that may be introduced into the estimated NO_3^- input by the depth of the defined nitracline. With a constant Kz in the 2 cases, they estimated that NO_3^- input was 5 times lower at the top of nitracline depth than at the maximum gradient depth. In our study, the NO_3^- input would also be ~ 3-4 times higher if calculated at the maximum gradient depth rather than at the top of the nitracline, mainly due to the increase of the nitracline gradient up to 48 µmol N m-4 (Table 1). However, in all cases, the NO_3^- input by turbulence always represented a minor contribution to the N budget.

Finally, the high contribution of N_2 fixation to new N input in the photic layer results from the intrinsically high N_2 fixation rates we measured in the WTSP (especially in MA waters), that are part of the hotspot of N_2 fixation reported by Bonnet et al., (2017), with rates being in the upper range of rates reported in the global N_2 fixation Marine Ecosystem Data (MAREDAT) database (Luo et al., 2012). Those high N_2 fixation rates are as high as westward in the Salomon Sea (Berthelot et al., 2017; Bonnet et al., 2015), extending the hotspot of N_2 fixation to the whole WTSP (Bonnet et al., 2017).”

P13, Line 13 — (and elsewhere) primary production or particulate phosphorus?

In this sentence, PP is used for primary production. We agree with this remark, thus we have change the PP of particulate phosphorus to POP.

P13, Lines 15-16 — correct these scientific notations for gene copies

"nifH copies L^{-1}" has been changed to "nifH gene copies L^{-1}" in the new version

P13, Lines 20-21 — Why use the areal rates here instead of the volumetric rates?

We used the areal rates to be consistent with the units of all the N inputs in the photic layer and N export, as reported in Table 4.

P13, Line 32 – P14, Line 2 — Awkward sentence. Rewrite for clarity.

It has been rewritten as follows: "The export efficiency of UCYN-B (2.3 % on average) and het-1 (4.0 % on average) was higher than that on of *Trichodesmium*, which is consistent with Bonnet et al., (2016b) and Karl et al., (2012). In a mesocosm experiment performed in the coastal waters of New Caledonia, Bonnet et al., (2016b) revealed that UCYN-C were efficiently exported thanks to aggregation processes."

P14, Lines 7-14 — Confusing text. Rewrite for clarity.

It has been rewritten as follows: "The contribution of diazotrophs to PC export at LD A (up to 30.6 % at 330 m) was high compared to what has been measured in a much smaller water column (15 m-high mesocosms) in New Caledonia (ca. 20 %; Bonnet et al, 2016a), and suggests that the direct export of diazotrophs should be further investigated in oligotrophic open waters. To date, few qPCR data on nifH from sediment traps are available (Karl et al., 2012) to compare with our study. However, it must be noted that we measured the highest export and e-ratio at LD A, where Trichodesmium dominated the diazotroph community. This suggests that most of the export was probably indirect, i.e. after the transfer of diazotroph-derived N (DDN) to the surrounding bacterial, phytoplankton and zooplankton communities, as revealed by Caffin et al., (this issue) during the same cruise, that are subsequently exported."

P14, Line 22 — What is "PCD"?

Programmed cell death (PCD) has been defined in the new version of the manuscript.

P16, Lines 12-26—The conclusion section is a little flat. The authors could do a better job of placing their study into a better context in terms of the global N budget and C export in the oceans.

We understand reviewer #4's comment. However, we have opened up the discussion of the fact that the oligotrophic ocean, which represents 60 % of the global ocean surface, could play a more significant role in C export than initially considered. We think that it is not consistent to perform larger C and N budget, at higher spatial scale, with the dataset that we have mentioned here at only 3 stations during one season of the year. Thus, to fill information and data gaps relative to our study and to enhance understanding of the global ocean, we think that a time-series study should be established in this region of the world ocean. This would give us the 'big picture' of the role of N2 fixation in export, as Bonnet et al. (2017) have shown that the WTSP is a hotspot of N2 fixation, and we have mentioned the important role of this process with regard to export, that is a major issue in oceanography today. In addition, this would complete the present time-series HOT, BATS and DYFAMED that were established in the North Pacific, North Atlantic Oceans and Mediterranean Sea, respectively.

Table 1 — Are these Kz values supposed to be in scientific notation? Are the units for the nitracline correct?

This has been corrected to "1.11 x 10^{-5} ± 1.00 x 10^{-5}" in the new version. We confirm that the units for the nitracline are correct.

Technical Corrections

P1, Line 24 — add "respectively" after "LD B"

This has been corrected in the new version

P1, Line 28 — add a comma after "LD C"

This has been corrected in the new version

P1, Line 31 — PC and PP not defined (or PN earlier)

This has been corrected in the new version

P1, Line 34 — change "there" to "their"

This has been corrected in the new version

P2, Line 5 — add comma after "ammonia"

It has been corrected in the new version

P2, Line 7 — "before" is an awkward word choice

P2, Lines 7-11 — Long, run on sentence. Rewrite for clarity.

This has been rewritten as follows: "In the oligotrophic ocean, N availability often limits phytoplankton growth (e.g. Moore et al., 2013) and N_2 fixation sustains a significant part of new primary production (PP, i.e. the production unrelated to internal recycling of organic matter in the photic layer) such as in the North (Karl et al., 1997) and South Pacific Ocean (Moutin et al., 2008), the western Mediterranean Sea (Garcia et al., 2006), or the tropical North Atlantic (Capone et al., 2005)."

P2, Line 21 — add comma after "...et al., 2008)"

This has been corrected in the new version

P2, Line 24 — add comma after "large"

This has been corrected in the new version

P2, Line 25 — add comma after "phytoplankton"

This has been corrected in the new version

P2, Line 29 — add comma after "ocean"

This has been corrected in the new version

P3, Line 9 — “harbouring” is an awkward word choice

This has been replaced by “including” in the new version

P3, Lines 10-15 — Long, run on sentence. Rewrite for clarity.

This has been rewritten as follows: “The region covered by the OUTPACE cruise is characterized by trophic and N_2 fixation gradients (Moutin et al., 2017). The westward oligotrophic waters are characterized by high N_2 fixation rates (631 ± 286 µmol N m^{-2} d^{-1}) mainly associated with *Trichodesmium* (i.e. within the hotspot around Melanesian archipelago waters, hereafter named MA), and the eastward ultra-oligotrophic waters (in the eastern border of the South Pacific gyre, hereafter named SPG waters) are characterized by low N_2 fixation rates (85 ± 79 µmol N m^{-2} d^{-1}) mainly associated with UCYN (Bonnet et al., this issue; Stenegren et al., this issue).

P3, Line 17 — add comma after “study”

This has been corrected in the new version

P4, Line 22 — “every day”

This has been corrected in the new version

P7, Line 13 — “weighed”

This has been corrected in the new version

P9, Line 13 — add “from” after “ranged”

It has been corrected in the new version

P10, Line 32 — “2.67 x 104”

This has been corrected in the new version

P11, Line 2 — change “from” to “for”

This has been corrected in the new version

The paper requires a thorough editing for grammar, word choice, and punctuation.

The new version of the manuscript has been reviewed and corrected by an English native speaker.

References

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